

34	1	While this analysis for the CMIP6 model set is accurate and focuses on a dataset that has shown higher ECS than previous iterations. it should be noted the models have exhibited considerable skill in predicting the subsequent GMST warming[22]. The criticism of the CMIP6 based high ECS models may be premature and probably will have to wait additional time before passing judgement. As noted in [22] these models have shown considerable skill when evaluated on their projections over the past 30-40 years after the projections were published. Figure 5.3 in the Report clearly refutes this assertion	[22]Hausfather, Z., Drake, H. F., Abbott, T., & Schmidt, G. A. (2020). Evaluating the performance of past climate model projections. Geophysical Research Letters, 47, e2019GL085378. https://doi.org/10.1029/2019GL085378
36	1	A recent study attributes the bias to inter-annual availability and residual bias in the satellite record. The pre- and post 2000 have temperature records from two separate satellites and the discrepancy in the trends needs to be resolved[23] Ref 23 is worth adding I think The Chung et al. study (which did not go beyond 2014) confirms the models' too-high warming rates as well as documenting the shift in tropospheric temperatures of the RSS and UW datasets (which share very similar choice of satellites) relative to UAH and NOAA/STAR (which rely on empirical tests to determine when a satellite drifts too much). Christy et al. 2018 specifically examined the relative jump in RSS and UW datasets around 2000 finding that compared with other independent measures, the RSS and UW retain spurious warming from the aging NOAA-14 satellite. Thus the notion that satellite biases are an issue applies predominantly to RSS and UW. We agree that the timing of major events like ENSOs diminishes the fidelity of models to observations comparisons, but now with 46 years of record in Fig. 5.7, this concern is less valid. Be that as it may, CGW Fig. 5.7 is clear in noting all models overestimate the tropical temperature trend - even more positive than that of RSS and the two Reanalyses.	[23]Chung, ES., Kim, SJ., Sohn, BJ. et al. Multi-decadal climate variability and satellite biases have amplified model-observation discrepancies in tropical troposphere warming estimates. Commun Earth Environ 5, 342 (2024). https://doi.org/10.1038/s43247-024-01510-8
38	3	This is challenge we need to face and work on improving the parametrizations. The new era of super high resolution models would resolve the complex interactions of turbulence, transport and clouds and will provide less uncertainty[24]. However, this can't be used to invalidate the entire set of models, the models are performing remarkably well given the complex system that is being modeled. It should be noted the models do cover the range of the observations and it is not outside the envelope of model predictions. The Report does not address invalidating the entire set of models. Rather we address the fitness-for-purpose of the models for policy making	[24]Merlis, T. M., Guendelman, I., Cheng, K.-Y., Harris, L., Chen, Y.-T., Bretherton, C. S., et al. (2024). The vertical structure of tropical temperature change in global storm-resolving model simulations of climate change. Geophysical Research Letters, 51, e2024GL111549. https://doi.org/10.1029/2024GL111549

40	4	Part of the problem here may be which part of the stratosphere is sampled and the importance of interannual a variabilities in the lower part of the stratosphere. [25] have shown that including the stratosphere as defined from 25 to 50km there is high confidence that the cooling signal caused by CO2 increase is detectable and beyond the noise levels of the natural and inter annual variability in the lowest part of the stratosphere. We should put this debate to rest. JOHN AND ROY? NEW SANTER PAPER NEEDS TO BE REFERENCED	[25] B.D. Santer, S. Po-Chedley, L. Zhao, C. Zou, Q. Fu, S. Solomon, D.W.J. Thompson, C. Mears, & K.E. Taylor, Exceptional stratospheric contribution to human fingerprints on atmospheric temperature, Proc. Natl. Acad. Sci. U.S.A. 120 (20) e2300758120, https://doi.org/10.1073/pnas.2300758120 (2023).
42	2	Given the large uncertainties in obtaining data over much of the Northern Hemisphere, using a smaller regional analysis to discount the models may be inappropriate. We should that this remains a question we need to resolve developing better datasets with more seasonal and sub-seasonal coverage to test the model on a global scale This section directly addresses a region that is important to estimates of the Social Cost of Carbon in the context of agricultural productivity, again reiterating that the models are not fit for the purpose of policy making	
44	3	This statement from [26] refers to the fitness of global scale models to resolve regional scale features and argues for developing model at higher spatial resolutions. Using downscaling and other techniques additional insights can be gained at regional scales and provide information for a set of 'purposes'. However, we should work on getting to regional and local scales with GCM as quickly as possible.	
47	2	Looking for trends or estimating trends in extreme events in current and future climate is not necessarily an attribution study. Estimating frequency, duration and intensity of events helps understand the changing range of the hazard (if there is one). An attribution would be to find to what extent a particular event is influenced or affected by increase in CO2. I would consider these two not to be the same.	
47	3	Attributing any single event to changing climate is perilous, agreed.	

47	5	As noted by [26] there is not yet any strong evidence that LPT is a fact	[26]Cohn, T. A., and H. F. Lins (2005), Nature's style: Naturally trendy, Geophysics. Res. Lett., 32, L23402, doi:10.1029/2005GL024476.
57	2	I am not sure how one can conclude that the temperature extremes have decreased based on figure 6.3.4. If I understand correctly the Tmax-Tmin for each year was tabulated and a mean of this difference averaged over the 100+ years was subtracted from these values to get some thing like a anomaly. This anomaly seems to have moved from +3F to -3F over the course of q00 years and now trending upwards again. This suggests to me that there has been significant changes in the extremes over the years and the most recent trend is towards warmer highs? For each station and each year, the hottest day and coldest night are determined and their differnce computed. These differences are then geographcially averaged for the CONUS and then an overall average is removed to show the results as a type of anomaly. Reviewer 1 is correct, as indicated in the text, Tmin temperatures are almost universally rising more rapidly than Tmax, hence, the difference (or range) has declined overall. The flattening (or slight rise in the metric) in the past 20 years is clearly within the natural range, but does reveal an interesting stair-step pattern with rapid declines centered around the 1940's and 1990's and relatively flat periods in between. An explanation is under investigation, and a leading hypothesis is that there was rapid growth of manufactured surfaces around weather stations in the 1940s. Then, in the 1960s-80s many stations were re-located to relatively rural airports, which then experienced subsequent growth in manufactured surfaces around their weather stations in the 1990s. Thus, rapid nighttime warming in the 1940s and 1990s.	
58	3	The NCA5 is suggesting that the western US is experiencing increase in extreme heat (page 57, para 4); it seems like figure 6.3.5 establishes the fact and not contradict. There was no claim of pervasive (all CONUS) increase in the report	
59	4	The definition of heat wave as consecutive 6 days seems to be a special case. The standard definition is exceedance of 85 or 90 percentile for 2 or more days. I can't check with NCA5 as it was taken down from the web As correctly suggested by Reviewer 1, there are numerous ways to define a heat wave. CWG chose to follow one defimtion discussed in NCA4 (pg 191) and use 6 consecutive days with Tmax > 90th percentile so to allow for consistent comparisons. However, changing the parameters will do little to alter the fundamental result that the 1920's to early 1950's contain the major heat wave events for the CONUS.	

60	1,2	Figure 6.3.6 establishes that heatwaves as defined here have increased in the west and have shown increasing trend all over CONUS in the past 50 years. [27] shows the results for the last 60 years and there is little doubt that there is an increase over most of the CONUS, again the NCA5 report is off the web; figure 6.3.7 shows the heat waves are monotonically increasing from the 1960. Including the longer data is only interesting in the dust bowl 1930 camouflage the signal somewhat for the Midwest and central plains. The problem of taking averages over large regions (for example entire CONUS) is that it kills the peak signals and smooths out the emergent climate change indicators. Doing continental averages to look for extremes is really not very useful. Secondly, there is no particular reason to assume that the climate change signal (i.e. the number of heat waves) should showing up 100 years ago, as the CO2 in the atmosphere started to be of significance in the last 50 years. As discussed in the CWG text, the EPA chart is (1) based on nighttime temperatures in the 50 largest US cities and (2) begins in 1961 which was the start of the coolest decade in over 100 years, thereby preconditioning the chart to have an upward trend. Urbanization is most clearly seen in nighttime temperature increases so this does not represent a change in the background climate, but more likely the local climates of cities. Rural areas show less nighttime warming. And avoiding earlier warm periods does not provide information on the larger scale of natural variability.	[27] https://www.epa.gov/climate-indicators/climate-change-indicators-heat-waves
61	4	averaged over all stations in the region, none of these trend parameters is statistically significant'. This is again how one would be looking at statistics of extremes in precipitation. As we know, precipitation is highly heterogeneous and location dependent. As observed there are trends even with the approaches chosen by the authors of this study compared to NCA5 and averaging that over and saying there is no trend seems disingenuous.	
62	2	same argument as above	
63	box	again the issue here is that we are talking about events that are happening in a short-time and over a small region. Averaging over time and space and develop statistics is probably not appropriate when we are talking about events such as the recent flooding event in Texas. If you average that event over 30 days, it will seem unremarkable. Extreme event totals are 1-day to 5-days depending on section discussed.	

64	1	As noted in NCA5 and shown in the analysis presented in figure 6.4.4 there is a clear trend of increase in extreme precipitation in 12 of the 27 stations analyzed. Again averaging over the entire region is not the best method for picking extreme precipitation events that are local both in time and space.	
69	2	However, there is evidence that flash droughts, which last weeks have increased over the Northeast and Midwest [28] The paper referenced by Reviewer 1 begins with data so recent (1999) that it cannot provide evidence for long-term changes in drought even though it suggests an increase in "flash droughts" over that period. Keep in mind, the DM is a product of subjective interpretation and objective measures - objective measures that have changed considerably during the past 25 years. (One of the CWG authors was an official contributor to the DM for 15 years.) One would wonder what would the 1920's, 30's and early 50's reveal if the DM was operational back then? A more consistent metric is provided in CWG Fig. 6.7.1.	[28]Ronald D. Leeper, Rocky Bilotta, Bryan Petersen, Crystal J. Stiles, Richard Heim, Brian Fuchs, Olivier P. Prat, Michael Palecki, Steve Ansari
70	2	More recent analysis indicate forest loss due to wildfire accelerating over the recent past[29]. In particular there has been an increase in extreme fire radiative power with a 2 fold increase from 2003 to 2023 (period of satellite data availability)[30][31]. However, that these fires are not out-of-sample and there is a fire deficit as shown in figure 6.8.2 should be acknowledged. But this doesn't alleviate the concern that wildfire is leading to lot of socio-economic concerns and needs to be addressed[32]	[29]Tyukavina Alexandra , Potapov Peter , Hansen Matthew C. , Pickens Amy H. , Stehman Stephen V. , Turubanova Svetlana , Parker Diana , Zalles Viviana , Lima André , Kommareddy Indrani , Song Xiao-Peng , Wang Lei , Harris Nancy; Global Trends of Forest Loss Due to Fire From 2001 to 2019, Frontiers in Remote Sensing.Volume 3 – 2022,2022. [30]Cunningham, C.X., Williamson, G.J. & Bowman, D.M.J.S. Increasing frequency and intensity of the most extreme wildfires on Earth. Nat Ecol Evol 8, 1420–1425 (2024). https://doi.org/10.1038/s41559-024-02452-2 [31]Patrick H. Freeborn, W. Matt Jolly, Mark A. Cochrane, Gareth Roberts, Large wildfire driven increases in nighttime fire activity observed across CONUS from 2003–2020, Remote Sensing of Environment, Volume 268, 2022,12777 [32]Parks, S.A., Guiterman, C.H., Margolis, E.Q. et al. A fire deficit persists across diverse North American forests despite recent increases in area burned. Nat Commun 16, 1493 (2025). https://doi.org/10.1038/s41467-025-56333-8
71	3	The question of forest management and its role on wildfire frequency and intensity is interesting and deserves further study.	

80	1	The global mean sea-level was increasing at a rate of 0.08 inches/yr in 1993 compared 0.18 inches/yr in 2023, almost double the rate[33], which is approximately what is needed to meet the NOAA projections by mid-century and seems plausible.	[33]Hamlington, B.D., Bellas-Manley, A., Willis, J.K. et al. The rate of global sea level rise doubled during the past three decades. Commun Earth Environ 5, 601 (2024). https://doi.org/10.1038/s43247-024-01761-5
88	3	A possible reason the method used by IPCC (optimal fingerprinting) can be considered to address McKitnick criticism is in [34]	[34]Chen, H., Chen, S.X. & Mu, M. A statistical review on the optimal fingerprinting approach in climate change studies. Clim Dyn 62, 1439–1446 (2024). https://doi.org/10.1007/s00382-023-06975-5
92	4	The role of clouds remains the single most uncertainty in climate modeling, I am glad this was acknowledged here.	
93	3	The substance of this paragraph was addressed in my comments in earlier sections. Some of the claims made here are either exaggerations of the usual practice (RCP8.5 being referred to as BAU) or the comment about not fit for purpose (a comment extracted from a publication that is calling out for cloud resolved models.	
105	2,3, 4,5	The problem with both the FACE studies and the soybean. Maize and wheat studies cited here are that they don't consider the effect of increase in temperature, changes in precipitation, humidity and other environmental factors along with CO2 increases. These compound effects have been shown in wide number of studies [35] [36][37][38][39]. A meta analysis of the compound effects shows that increase in CO2, along with temperature, precipitation changes effects the quality of the cereal and yields[40]. Another recent study evaluated the impacts on C3 and C4 crops and conclude that CO2 enrichment for C3 crops could increase but will be compensated by temperature increase. CO2 fertilization effect was found to be small for C4 [41]. It is important therefore that we consider the compound effects on crop yields and not just focus on CO2 enrichment.	[35] Ruiz-Vera UM, Siebers M, Gray SB, Drag DW, Rosenthal DM, Kimball BA, Ort DR, Bernacchi CJ. Global warming can negate the expected CO2 stimulation in photosynthesis and productivity for soybean grown in the Midwestern United States. Plant Physiol. 2013 May;162(1):410-23 [36] Levine LH, Richards JT, Wheeler RM. Super-elevated CO2 interferes with stomatal response to ABA and night closure in soybean (Glycine max). J Plant Physiol. 2009 Jun 1;166(9):903-13. [37]Večeřová K, Oravec M, Puranik S, Findurová H, Veselá B, Opoku E, Ofori-Amanfo KK, Klem K, Urban O, Sahu PP. Single and interactive effects of variables associated with climate change on wheat metabolome. Front Plant Sci. 2022 Oct 10;13:1002561. [38]Thomas, J.M.G., Boote, K.J., Allen, L.H., Gallo-Meagher, M. and Davis, J.M. (2003), Elevated Temperature and Carbon Dioxide Effects on Soybean Seed Composition and Transcript Abundance. Crop Sci., 43: 1548-1557. https://doi.org/10.2135/cropsci2003.1548 [39] Lesk, C., Anderson, W., Rigden, A. et al. Compound heat and moisture extreme impacts on global crop yields under climate change. Nat Rev Earth Environ 3, 872–889 (2022). https://doi.org/10.1038/s43017-022-00368-8 [40] Ben Mariem S, Soba D, Zhou B, Loladze I, Morales F, Aranjuelo I. Climate Change, Crop Yields, and Grain Quality of C3 Cereals: A Meta-Analysis of [CO2], Temperature, and Drought Effects. Plants. 2021; 10(6):1052. https://doi.org/10.3390/plants10061052

			[41]Rezaei, E.E., Webber, H., Asseng, S. et al. Climate change impacts on crop yields. Nat Rev Earth Environ 4, 831–846 (2023). https://doi.org/10.1038/s43017-023-00491-0
106	1	Taylor and Schlenker 2021 also note that 'Climate change will likely have negative impact on agriculture in aggregate, especially in regions exposed to extreme heat. And the CO2-driven yield increases may be offset by effects on food nutrition and quality'. However, the results for US have been a net positive in the recent past but not true in the lower latitudes.	
111	1,2	The criticism of NOAA Billion dollar map is likely warranted as it was never completely transparent how the data was generated. However, changing the baseline assumption and refactoring the data using a different set of criteria to make the effects smaller also doesn't make sense. We should continue collecting this data and make the data accessible for researchers and peer review. Figure 10.1 doesn't address these issues, it is another methodology with unknown provenance of the data.	
117	3	Climate change can be expected to increase cold weather extremes in regions that have not been exposed to these effects	

118	3	The same Nobel prize winning model in its most recent study concludes that [42] "We conclude with three results relevant for climate policy. First, both current policies (base run) and the extended Paris Accord fall short of limiting global warming to 2 °C or to the cost–benefit optimal level. Second, the economic stakes in global climate policy are substantial, with estimated net present value of economic benefits around \$120 trillion from the cost–benefit optimal policy. Third, once differences in discounting are considered, the baseline DICE-2023 estimate of the social cost of carbon (\$66/tCO2 for 2020) aligns closely with other recent estimates". Obviously this is the latest and the current version of the model and these results are quite different from thus discussed on this page. These conclusions should also be included in this discussion to avoid selective reading of the literature.	[42]L. Barrage, & W. Nordhaus, Policies, projections, and the social cost of carbon: Results from the DICE-2023 model, Proc. Natl. Acad. Sci. U.S.A. 121 (13) e2312030121, https://doi.org/10.1073/pnas.2312030121 (2024).
120	1	Similar empirical studies [43] point to "Using an empirical approach that provides a robust lower bound on the persistence of impacts on economic growth, we find that the world economy is committed to an income reduction of 19% within the next 26 years independent of future emission choices (relative to a baseline without climate impacts, likely range of 11–29% accounting for physical climate and empirical uncertainty). These damages already outweigh the mitigation costs required to limit global warming to 2 °C by sixfold over this near-term time frame and thereafter diverge strongly dependent on emission choices. Committed damages arise predominantly through changes in average temperature, but accounting for further climatic components raises estimates by approximately 50% and leads to stronger regional heterogeneity. Committed losses are projected for all regions except those at very high latitudes, at which reductions in temperature variability bring benefits. The largest losses are committed at lower latitudes in regions with lower cumulative historical emissions and lower present-day income". This study uses more of the daily temperature precipitation from 1600 hundred locations world wide instead of global means. There are numerous studies that show the cost, many described in the COP 21 and follow-on meetings	[43] Kotz, M., Levermann, A. & Wenz, L. The economic commitment of climate change. Nature 628, 551–557 (2024). https://doi.org/10.1038/s41586-024-07219-0

Reviewer #1 JRC comments. No need to add citation

36	1	A recent study attributes the bias to inter-annual availability and residual bias in the satellite record. The pre- and post 2000 have temperature records from two separate satellites and the discrepancy in the trends needs to be resolved[23] Ref 23 is worth adding I think Chung et al. 2024 (using output through 2014 only) confirm the models' too-high tropical tropospheric warming rates relative to observations and suggested two issues that might contribute to at least part of the discrepancy: (1) the differential phasing of natural, ephemeral events such as El Niños between models and observations and (2) a possible spurious cooling in UAH and NOAA/STAR satellite datasets around 2000. Regarding (1) we agree that the timing of major events like ENSOs diminishes the confidence of models-to-observations comparisons for short periods, but now with 46 years of record as well as two major El Niño warm events since 2014 in the observations, the results shown in Fig. 5.7 provide a more confident comparison as indicated by statistical significance results. Regarding (2) Christy et al. 2018 (and later Zou et al 2023) specifically examined the divergence around 2000 between UAH and NOAA/STAR vs. RSS and UW. We demonstrated that RSS and UW's use of the aging and drifting NOAA-14 MSU sensor relative to the new, highly calibrated NOAA-15 AMSU sensor contributed to a spurious warming in RSS and UW. Be that as it may, CWG Fig. 5.7 is clear in noting all models overestimate the tropical temperature trend relative to observations – model trends being more positive than observational datasets - even RSS, as well as two Reanalyses and the UAH and NOAA/STAR satellite products. Do we need to add ref [23] and a sentence to the text? I'll defer to John John has done the most detailed work on this, and don't the various satellite datasets now mostly agree with each other, especially in the tropics?
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Reviewer #6. JRC comments

6	57	para 1	The conclusion "The rise in Tmin has been strongly related to the growing presence of manufactured surfaces around the weather stations over the last 100+ years (the so-called urban heat island effect; Section 3.3 and Spencer et al. 2025)." is only found in an author's self-citation of Spencer, et al, (2025) that does not appear in the references at the end of Chapter 6. Further, the trend of greater increases in Tmin relative to Tmax is documented and explained in Easterling, et al (1997). https://doi.org/10.1126/science.277.5324.364; Thorne, et al (2016) https://doi.org/10.1002/2015JD024584. The trend cannot be attributed to UHI effects.
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			The citation of Thorne et al. 2016 is curious as there was no attempt to categorize data by urban size, since the authors utilized gridded (not station) data for their analysis – in fact the word “urban” does not appear in the article. There are many empirical studies which document the relative rise of TMin vs. TMax in urban areas, among the more established are Karl et al. 1988 and Runnalls and Oke 2006 who state about these effects, “differences of thermal microclimate are enhanced at night” using the simple metric (TMax – TMin). Spencer et al 2025 references many publications which also point to the same conclusion, and more importantly demonstrate that the UHI effect has its greatest impact when a site changes from extreme rural to less rural (as did Oke 1973). These results render Easterling et al. 1997 uninformative because their “nonurban” definition included cities with as many as 50,000 inhabitants, and thus their “nonurban” sample was heavily contaminated by UHI. As Spencer et al. 2025 show, the greatest impact occurs as populations increase from near zero to this level. The physics of the issue are discussed in Christy et al. 2009 and theoretically modeled in McNider et al. 2012 (see citations therein for other studies).
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Include citations to Christy et al. 2009 (already in the list), for the increase in TMin relative to TMax in cities.

Karl, T.R., H.F. Diaz, and G. Kukla. 1988: Urbanization: Its detection and effect on the United States climate record. *J. Climate*, 1, 1099-1123. [https://doi.org/10.1175/1520-0442\(1988\)001<1099:UIDAEI>2.0.CO;2](https://doi.org/10.1175/1520-0442(1988)001<1099:UIDAEI>2.0.CO;2)

Runnalls, K.E. and T.R. Oke, 2006: Technique to detect microclimate inhomogeneities in historical records of screen-level air temperature. *J. Climate*, 19, 959-978. <https://doi.org/10.1175/JCLI3663.1>

McNider, R.T., G.J. Steeneveld, A.A.M. Holtslag, R.A. Pielke Sr., S. Mackaro, A. Pour-Biazar, J. Walters, U. Nair, and J.R. Christy, **2012**: Response and sensitivity of the nocturnal boundary layer over land to added longwave radiative forcing. *J. Geophys. Res.*, 117, D14106, doi:10.1029/2012JD017578.

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Comment or Suggested Edit

The greening effect can't be due to increase in CO₂ alone. The fertilization effect needs Nitrogen and phosphorous. There is observational[1] evidence that nitrogen available to plants is decreasing with increasing CO₂ and thus acting to limit fertilization effect. Secondly, the greening of some portions of the planet are likely due to changes in precipitation patterns associated with changing climatic conditions and not entirely attributable to fertilization effect. We will also need to consider that the temperature effect on photosynthesis will also be a limiting mechanism to any continued greening due to fertilization in the future [2]. I recommend acknowledging that the greening is not attributable to CO₂ alone and continued greening in the future with increasing CO₂ is uncertain.

I am glad we acknowledge that CO₂ causes warming, though I am not sure why we are not considering other energy sector related emissions that also cause warming (CFCs/HFCs, methane, aerosol effects). It is not clear what is meant by "CO₂ emissions have tended to overestimate observed trends". This is not correct, the cumulative emissions track the RCP8.5 scenario the closest [3,4]. While there was a decrease in emissions during the Covid-19 period the emissions are now back on track following RCP8.5. This statement should clarify what is meant by the statement in the text.

This paragraph seems to be talking about equilibrium climate sensitivity. The most recent model inter-comparison gives a wider range of climate sensitivities in the range of 1.8 to 5.7 and is wider than estimates of climate sensitivities that have been estimated from paleoclimatic and more recent historical datasets, in the range could be in the range of 2.3 to 4.5 K[5]. The higher sensitivity in the most recent model assessments are an area of active investigation by modelers, the feasibility of a higher range needs to be confirmed with additional observational and process scale modeling [6]. The range for climate sensitivities, 1.8-2.7, should be corrected to the most recent findings, 2.3-4.5. We should also recognize that the (a)earth is a complex system of systems (b)we can't do an experiment to find out what is correct or reduced the uncertainty. As we get better at extracting temperature records and CO₂ concentrations from paleo data, there will be better reconstructions and possibly narrower range along with improvements in modeling that resolve many physical, biological and chemical processes as we get to higher and cloud resolving spatial resolutions.

This is again not true for the majority of the models in the AR6. Figure 7.19 in the IPCC AR6 report shows that majority of the models exhibit a range within the observational range. The models running hot in the most recent decades are the ones with climate sensitivities greater than 5K, which are 3 out of a total of 40 and it should be noted that about the same number of models also are running cold' compared to observed[7]

There is very little dispute that these are challenging issues for climate science. Many of the issues identified here related to compound effects (subsidence and sea-level rise; wildfire and forest management) that requires further scientific investigation. Identifying further research avenues/focus areas will be helpful to firm up some of the challenges faced in using the climate projections as discussed in the paragraph 9.

The focus on uncertainty is relevant and should be emphasized in future research. However, as a reasonable policy pathways, making decisions in the face of these uncertainties is critical. Most of the climate uncertainties that we have confidence at this time can be considered Level 2 and Level 3 uncertainties, approaches such as scenario analysis (and others) are suggested and followed for making hazard and risk estimates for future conditions. For deep uncertainty hazards (for example tornadoes etc.) that we don't have enough data or capability to model, developing methods for assessing the uncertainty and risk should be a focus of future research.

The reason CO₂ was identified as a pollutant is the health consequences of increased temperature, flooding and other climate impacts and not because of any direct affect on health due to exposure to atmospheric CO₂[8].

The summary should note that CO₂ fertilization effects are limited by the availability of other critical plant nutrients (Nitrogen, Phosphorous). The fertilization effect is also constrained by higher temperature effecting photosynthesis[9]

The attribution of greening to primarily CO₂ fertilization is questionable as per recent research. Specifically, in the Northern high latitudes (where the greening is most prominent), increases in temperatures due to climate change has been shown to provide the best explanation[10]. In the sub-Saharan region reforestation efforts and changing precipitation patterns lead to some of the observed greening (the great green wall) [11]. The attribution to CO₂ fertilization should be qualified by noting the other present and emerging constraints.

This summarization ignores the compound effects of temperature, moisture and CO₂ fertilization in assessing the impacts of CO₂ increase on vegetation and crop yields. Many studies have shown that [12]. Making note of these compensating factors is required for a balanced view on CO₂ fertilization effects on plants.

Chen et al. (2024)[13] conclude that "In the attribution analysis of LAI trend and growth rate trend, there were some differences between multiple linear regression model and partial correlation analysis at high latitudes in the northern hemisphere. Partial correlation analysis showed that LAI trend and growth rate trend in this region were mainly affected by temperature and radiation, while multiple linear regression suggested that CO₂ was the dominant driver. The results of the partial correlation analysis seem more reasonable because some previous studies have shown that vegetation change in the high latitudes of the northern Hemisphere is mainly positively affected by climate warming. Multiple linear regression model performed relatively poorly in this region and might not fully identify the contribution of individual drivers. In addition, our model does not include the contribution of land management, which has been highlighted in some previous studies. Of course, the contribution of land management to LAI trend and growth rate trend may be implied in other drivers, such as the positive contribution of CO₂ to LAI trend in PP and PN areas".

This again suffers from not acknowledging the effects of compound and often compensating phenomena that is occurring with the on-going climate change. For example [14] note that much of the Midwest any benefit from CO₂ fertilization will be negated by temperature increases. [15] note that "Our results show that long-term physiological acclimation dampens the initial stimulation of plant net carbon assimilation to elevated [CO₂], and of plant water use to warming."

The fallacy of these arguments in terms of PH changes and evolution is that the changes described in this paragraph occurred over millennia and the evolutionary process had time to adapt; here with climate change we are talking about changes in our lifetime, the speed of change is the issue. This will lead to collapse of some ecosystem and by the time adaptation takes place it will be millions of years and in a different climate - not the one we have now and that enabled the rise of humans.

A simple calculation will show us how misleading this figure is. It shows that over 600 Million years CO₂ changed by around 6000ppm. That is a rate of change of 0.00001/yr; CO₂ changed from 275 to 430 in the past 175 years, that is rate of change of 0.88ppm/yr. A change in the rate of over 10⁵ in magnitude - there in lies the problem. We are seeing a rapid increase in CO₂ that has never been witnessed and the ecosystem and human society (which is at best 10K years old), may not be able to adapt to this rapid change.

The misuse of RCP8.5 as business as usual is acknowledged. However, this was never intended to be a no-policy or business as usual scenario but as a high-end baseline scenario. When dealing with interaction of physical systems with socio-economic projections (which tend to be highly uncertain and policy dependent), the development of this high baseline scenarios is intended to explore the uncertainty ranges that represent a high-end scenario[16,17]. As any engineer would acknowledge, this is a prudent approach when dealing with L3 and L4 type deep uncertainties and provide valid envelopes for assessing uncertainties and risks from high impact events/hazards. The rest of the statements here are uncertainties acknowledged by the climate scientists and included in developing projections and discussing the results[4]. Does the IPCC downplay the role of sun in climate change? The IPCC includes sun variability in all its assessments from the beginning and has been acknowledged as a potential but insignificant contributor.

Please see above. The business-as-usual may be a misnomer but this is an 95% case that is typically included in any type of prudent uncertainty analysis, in particular given the difficulty of predicting the evolution of socio-economic and political conditions over the next 100 years. It should be noted the difference between RCP8.5 and RCP4.5 for mid-century conditions that is of particular focus for adaptation studies is not significant and most of the divergence in these two scenarios occurs later in the century. That the dynamic vegetation models need more development is acknowledged and is a defining challenge for Earth System Modeling for the next decade.

As noted, Parker[18,19,20] has investigated the Urban effects on temperature records. The finding establish that by comparing warm and wind conditions world wide that the impact is negligible. However, as noted by authors, this literature hasn't progressed much over the last decade and much more attention needs to be paid to this question by employing newer models, datasets and analytical techniques available to us at present.

We can all agree that there is a desperate need for developing higher-resolution climate models that can get to cloud resolving scale so that we can represent the cloud feedbacks correctly in the model and avoid uncertainties from parameterization and scale dependency of the parameters. This doesn't make these models useless. The models provide directions for further improvements and necessary changes to reduce the uncertainty bands over the ECS.

The databased approaches have mostly been robust with changes at the high end around 3-4C and at the low end around 1.8 to 2.5C as noted in the chapter. It will be challenging to make this range shorter given the data requirements and uncertainties in data over paleo/haloacene periods. It seems reasonable to estimate that we may have ECS in the range of 3 to 4K (higher than 3.5K being less likely [21]) at the higher end and this should not prevent us from planning and thinking on ways to prevent this possible scenario as it could have major consequences for humans across the planet.

The models range over a factor of three in ECS comparing the most sensitive to the least sensitive models. However vast majority of the models are in 3 to 3.5K sensitivity range and it is probably not correct to assign all models the same amount of importance, given the fact that some of institutes participating in the exercise have different levels of capacity to develop and test models.

While this analysis for the CMIP6 model set is accurate and focuses on a dataset that has shown higher ECS than previous iterations. it should be noted the models have exhibited considerable skill in predicting the subsequent GMST warming[22]. The criticism of the CMIP6 based high ECS models may be premature and probably will have to wait additional time before passing judgement. As noted in [22] these models have shown considerable skill when evaluated on their projections over the past 30-40 years after the projections were published.

A recent study attributes the bias to inter-annual availability and residual bias in the satellite record. The pre- and post 2000 have temperature records from two separate satellites and the discrepancy in the trends needs to be resolved[23]

This is challenge we need to face and work on improving the parametrizations. The new era of super high resolution models would resolve the complex interactions of turbulence, transport and clouds and will provide less uncertainty[24]. However, this can't be used to invalidate the entire set of models, the models are performing remarkably well given the complex system that is being modeled. It should be noted the models do cover the range of the observations and it is not outside the envelope of model predictions.

Part of the problem here may be which part of the stratosphere is sampled and the importance of interannual variabilities in the lower part of the stratosphere. [25] have shown that including the stratosphere as defined from 25 to 50km there is high confidence that the cooling signal caused by CO2 increase is detectable and beyond the noise levels of the natural and inter annual variability in the lowest part of the stratosphere. We should put this debate to rest.

Given the large uncertainties in obtaining data over much of the Northern Hemisphere, using a smaller regional analysis to discount the models may be inappropriate. We should that this remains a question we need to resolve developing better datasets with more seasonal and sub-seasonal coverage to test the model on a global scale

This statement from [26] refers to the fitness of global scale models to resolve regional scale features and argues for developing model at higher spatial resolutions. Using downscaling and other techniques additional insights can be gained at regional scales and provide information for a set of 'purposes'. However, we should work on getting to regional and local scales with GCM as quickly as possible.

Looking for trends or estimating trends in extreme events in current and future climate is not necessarily an attribution study. Estimating frequency, duration and intensity of events helps understand the changing range of the hazard (if there is one). An attribution would be to find to what extent a particular event is influenced or affected by increase in CO2. I would consider these two not to be the same. Attributing any single event to changing climate is perilous, agreed.

As noted by [26] there is not yet any strong evidence that LPT is a fact

I am not sure how one can conclude that the temperature extremes have decreased based on figure 6.3.4. If I understand correctly the Tmax-Tmin for each year was tabulated and a mean of this difference averaged over the 100+ years was subtracted from these values to get some thing like a anomaly. This anomaly seems to have moved from +3F to -3F over the course of q00 years and now trending upwards again. This suggests to me that there has been significant changes in the extremes over the years and the most recent trend is towards warmer highs?

The NCA5 is suggesting that the western US is experiencing increase in extreme heat (page 57, para 4); it seems like figure 6.3.5 establishes the fact and not contradict. There was no claim of pervasive (all CONUS) increase in the report

The definition of heat wave as consecutive 6 days seems to be a special case. The standard definition is exceedance of 85 or 90 percentile for 2 or more days. I can't check with NCA5 as it was taken down from the web

Figure 6.3.6 establishes that heatwaves as defined here have increased in the west and have shown increasing trend all over CONUS in the past 50 years. [27] shows the results for the last 60 years and there is little doubt that there is an increase over most of the CONUS, again the NCA5 report is off the web; figure 6.3.7 shows the heat waves are monotonically increasing from the 1960. Including the longer data is only interesting in the dust bowl 1930 camouflage the signal somewhat for the Midwest and central plains. The problem of taking averages over large regions (for example entire CONUS) is that it kills the peak signals and smooths out the emergent climate change indicators. Doing continental averages to look for extremes is really not very useful. Secondly, there is no particular reason to assume that the climate change signal (i.e. the number of heat waves) should showing up 100 years ago, as the CO2 in the atmosphere started to be of significance in the last 50 years.

averaged over all stations in the region, none of these trend parameters is statistically significant'. This is again how one would be looking at statistics of extremes in precipitation. As we know, precipitation is highly heterogeneous and location dependent. As observed there are trends even with the approaches chosen by the authors of this study compared to NCA5 and averaging that over and saying there is no trend seems disingenuous.

same argument as above

again the issue here is that we are talking about events that are happening in a short-time and over a small region. Averaging over time and space and develop statistics is probably not appropriate when we are talking about events such as the recent flooding event in Texas. If you average that event over 30 days, it will seem unremarkable.

As noted in NCA5 and shown in the analysis presented in figure 6.4.4 there is a clear trend of increase in extreme precipitation in 12 of the 27 stations analyzed. Again averaging over the entire region is not the best method for picking extreme precipitation events that are local both in time and space.

However, there is evidence that flash droughts, which last weeks have increased over the Northeast and Midwest [28]

More recent analysis indicate forest loss due to wildfire accelerating over the recent past[29]. In particular there has been an increase in extreme fire radiative power with a 2 fold increase from 2003 to 2023 (period of satellite data availability)[30][31]. However, that these fires are not out-of-sample and there is a fire deficit as shown in figure 6.8.2 should be acknowledged. But this doesn't alleviate the concern that wildfire is leading to lot of socio-economic concerns and needs to be addressed[32]

The question of forest management and its role on wildfire frequency and intensity is interesting and deserves further study.

The global mean sea-level was increasing at a rate of 0.08 inches/yr in 1993 compared 0.18 inches/yr in 2023, almost double the rate[33], which is approximately what is needed to meet the NOAA projections by mid-century and seems plausible.

A possible reason the method used by IPCC (optimal fingerprinting) can be considered to address McKitnick criticism is in [34]

The role of clouds remains the single most uncertainty in climate modeling, I am glad this was acknowledged here.

The substance of this paragraph was addressed in my comments in earlier sections. Some of the claims made here are either exaggerations of the usual practice (RCP8.5 being referred to as BAU) or the comment about not fit for purpose (a comment extracted from a publication that is calling out for cloud resolved models.

The problem with both the FACE studies and the soybean. Maize and wheat studies cited here are that they don't consider the effect of increase in temperature, changes in precipitation, humidity and other environmental factors along with CO2 increases. These compound effects have been shown in wide number of studies [35] [36][37][38][39]. A meta analysis of the compound effects shows that increase in CO2, along with temperature, precipitation changes effects the quality of the cereal and yields[40]. Another recent study evaluated the impacts on C3 and C4 crops and conclude that CO2 enrichment for C3 crops could increase but will be compensated by temperature increase. CO2 fertilization effect was found to be small for C4 [41]. It is important therefore that we consider the compound effects on crop yields and not just focus on CO2 enrichment.

Taylor and Schlenker 2021 also note that 'Climate change will likely have negative impact on agriculture in aggregate, especially in regions exposed to extreme heat. And the CO2-driven yield increases may be offset be effects on food nutrition and quality'. However, the results for US have been a net positive in the recent past but not true in the lower latitudes.

The criticism of NOAA Billion dollar map is likely warranted as it was never completely transparent how the data was generated. However, changing the baseline assumption and refactoring the data using a different set of criteria to make the effects smaller also doesn't make sense. We should continue collecting this data and make the data accessible for researchers and peer review. Figure 10.1 doesn't address these issues, it is another methodology with unknown province of the data.

Climate change can be expected to increase cold weather extremes in regions that have not been exposed to these effect

The same Nobel prize winning model in its most recent study concludes that [42] "We conclude with three results relevant for climate policy. First, both current policies (base run) and the extended Paris Accord fall short of limiting global warming to 2 °C or to the cost–benefit optimal level. Second, the economic stakes in global climate policy are substantial, with estimated net present value of economic benefits around \$120 trillion from the cost–benefit optimal policy. Third, once differences in discounting are considered, the baseline DICE-2023 estimate of the social cost of carbon (\$66/tCO₂ for 2020) aligns closely with other recent estimates". Obviously this is the latest and the current version of the model and these results are quite different from thus discussed on this page. These conclusions should also be included in this discussion to avoid selective reading of the literature.

Similar empirical studies [43] point to "Using an empirical approach that provides a robust lower bound on the persistence of impacts on economic growth, we find that the world economy is committed to an income reduction of 19% within the next 26 years independent of future emission choices (relative to a baseline without climate impacts, likely range of 11–29% accounting for physical climate and empirical uncertainty). These damages already outweigh the mitigation costs required to limit global warming to 2 °C by sixfold over this near-term time frame and thereafter diverge strongly dependent on emission choices. Committed damages arise predominantly through changes in average temperature, but accounting for further climatic components raises estimates by approximately 50% and leads to stronger regional heterogeneity. Committed losses are projected for all regions except those at very high latitudes, at which reductions in temperature variability bring benefits. The largest losses are committed at lower latitudes in regions with lower cumulative historical emissions and lower present-day income". This study uses more of the daily temperature precipitation from 1600 hundred locations world wide instead of global means. There are numerous studies that show the cost, many described in the COP 21 and follow-on meetings

while small in terms of global emissions (just from the US transport sector), over period this will be significant reduction. We need to remember that US is the second highest emitter of CO₂.

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Francesco S.R. Pausata, Marco Gaetani, Gabriele Missouri, Alexis Berg, Danielle Maia de Souza, Rowan F. Sage, Peter B. deMenocal, The Greening of the Sahara: Past Changes and Future Implications, *One Earth*, Volume 2, Issue 3, 2020, Pages 235-250, ISSN 2590-3322, <https://doi.org/10.1016/j.oneear.2020.03.002>.

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[14] Ursula M. Ruiz-Vera, Matthew Siebers, Sharon B. Gray, David W. Drag, David M. Rosenthal, Bruce A. Kimball, Donald R. Ort, Carl J. Bernacchi, Global Warming Can Negate the Expected CO₂ Stimulation in Photosynthesis and Productivity for Soybean Grown in the Midwestern United States, *Plant Physiology*, Volume 162, Issue 1, May 2013, Pages 410–423, <https://doi.org/10.1104/pp.112.211938> [15] Lamba S, Hall M,

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The Chung et al. study (which did not go beyond 2014) confirms the models' too-high warming rates as well as documenting the shift in tropospheric temperatures of the RSS and UW datasets (which share very similar choice of satellites) relative to UAH and NOAA/STAR (which rely on empirical tests to determine when a satellite drifts too much). Christy et al. 2018 specifically examined the relative jump in RSS and UW datasets around 2000 finding that compared with other independent measures, the RSS and UW retain spurious warming from the aging NOAA-14 satellite. Thus the notion that satellite biases are an issue applies predominantly to RSS and UW. We agree that the timing of major events like ENSOs diminishes the fidelity of models to observations comparisons, but now with 46 years of record in Fig. 5.7, this concern is less valid. Be that as it may, CGW Fig. 5.7 is clear in noting all models overestimate the tropical temperature trend - even more positive than that of RSS and the two Reanalyses. [23] Chung, ES., Kim, SJ., Sohn, BJ. et al. Multi-decadal climate variability and satellite biases have amplified model-observation discrepancies in tropical troposphere warming estimates. *Commun Earth Environ* 5, 342 (2024). <https://doi.org/10.1038/s43247-024-01510-8>

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For each station and each year, the hottest day and coldest night are determined and their difference cc

As correctly suggested by Reviewer 1, there are numerous ways to define a heat wave. CWG chose to

As discussed in the CWG text, the EPA chart is (1) based on nighttime temperatures in the 50 largest l

Extreme event totals are 1-day to 5-days depending on section discussed.

The paper referenced by Reviewer 1 begins with data so recent (1999) that it cannot provide evidence for long-term changes in drought even though it suggests an increase in "flash droughts" over that period. Keep in mind, the DM is a product of subjective interpretation and objective measures - objective measures that have changed considerably during the past 25 years. (One of the CWG authors was an official contributor to the DM for 15 years.) One would wonder what would the 1920's, 30's and early 50's reveal if the DM was operational back then? A more consistent metric is provided in CWG Fig. 6.7.1.

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C., Anderson, W., Rigden, A. et al. Compound heat and moisture extreme impacts on global crop yields under climate change. *Nat Rev Earth Environ* 3, 872–889 (2022).

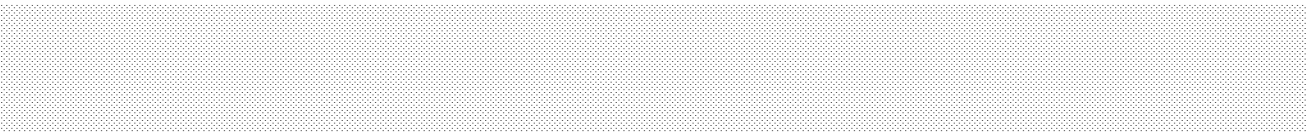
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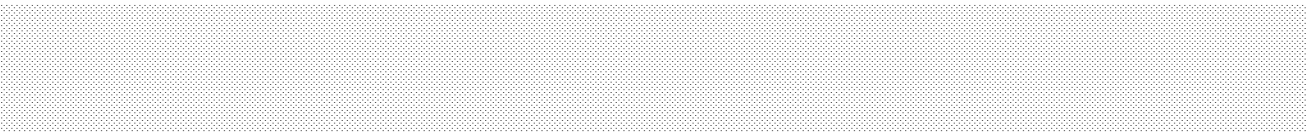


te Group Inc., 2024.

omputed. These differences are then geographcially averaged for the CONUS and then an overall ε

o follow one defimtion discussed in NCA4 (pg 191) and use 6 consecutive days with Tmax > 90th pe

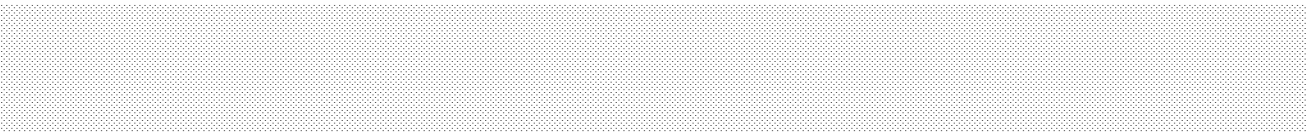
JS cities and (2) begins in 1961 which was the start of the coolest decade in over 100 years, thereby



average is removed to show the results as a type of anomaly. Reviewer 1 is correct, as indicated in

percentile so to allow for consistent comparisons. However, changing the parameters will do little to e

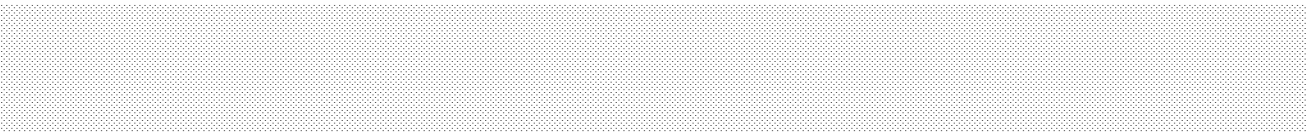
preconditioning the chart to have an upward trend. Urbanization is most clearly seen in nighttime te



the text, Tmin temperatures are almost universally rising more rapidly than Tmax, hence, the difference

alter the fundamental result that the 1920's to early 1950's contain the major heat wave events for the

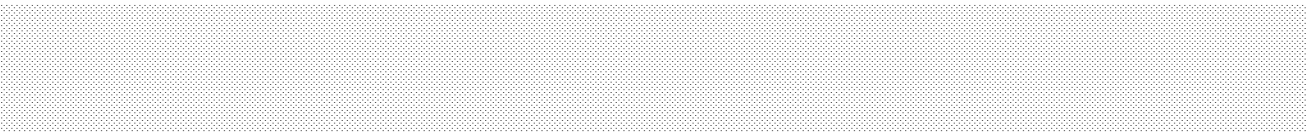
temperature increases so this does not represent a change in the background climate, but more likely



nce (or range) has declined overall. The flattening (or slight rise in the metric) in the past 20 years is

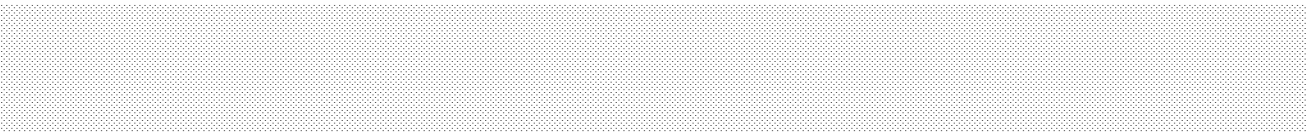
e CONUS.

/ the local climates of cities. Rural areas show less nighttime warming. And avoiding earlier warm p



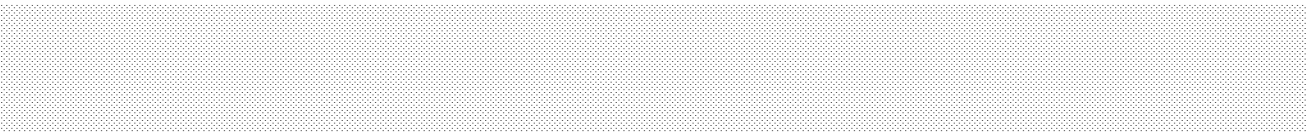
s clearly within the natural range, but does reveal an interesting stair-step pattern with rapid declines

periods does not provide information on the larger scale of natural variability. [27]<https://www.epa.gov>

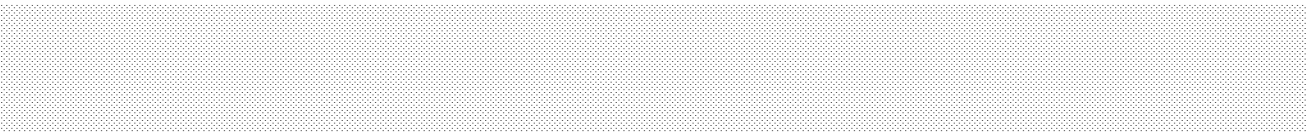


; centered around the 1940's and 1990's and relatively flat periods in between. An explanation is ur

v/climate-indicators/climate-change-indicators-heat-waves



nder investigation, and a leading hypothesis is that there was rapid growth of manufactured surfaces



; around weather stations in the 1940s. Then, in the 1960s-80s many stations were re-located to rel



latively rural airports, which then experienced subsequent growth in manufactured surfaces around



their weather statio

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Comment or Suggested Edit

This chapter points out that the primary concern around human emissions of CO₂ is that it changes the radiative balance of the atmosphere. The chapter also makes the points that CO₂ has no toxicological effects on humans at current atmospheric levels, and that CO₂ is necessary for photosynthesis, with the potential to stimulate plant growth at higher concentrations. These points are well-established scientific facts.

It would be more reflective of the literature to say here that "Many studies over the past decade have confirmed a global greening pattern (increase in LAI) attributable to rising CO₂ levels, changes in climate, changes in nitrogen deposition, land cover change, and other factors."

While the Zhu et al. 2016 study did find that 70.1% (+/- 29.4%) of the integrated global change in LAI was attributable to changes in CO₂, most of the observed change in LAI is concentrated in the tropical forest regions of South America, Africa, and Indonesia, as shown in this report's Figure 2.1, reproduced from Zhu et al. In terms of the fraction of global land area influenced most by the factors considered in Zhu et al. 2016, effects on LAI from changing climate is the dominant factor (positive effects for 28.4% of land area plus negative effects on 4.7%, for a total area with dominant climate influence of 33.1%), followed by other factors not represented explicitly in the study or in the models (positive effects on LAI from other factors over 25.0% of land area plus negative effects over 5.3%, for a total area with other factors as the dominant effect of 30.3%), and then CO₂ effects (23.2% of land area dominated by positive effects and 0.1% with negative effects, for a total of 23.4%). Since the report is focused on the U.S. it would be helpful to indicate the global context for the numbers reported. The dominant factors related to changes in LAI over the U.S. (including Alaska) are land cover change (+LCC), climate effects (+CLI), other factors (+OF and -OF), and then CO₂ (+CO₂). There are more extensive regions where +CO₂ is the dominant factor for the boreal forest zone in Canada. Given that, and given the explicit focus of the report on the U.S., it would be useful to call out in this section that the global greening pattern has distinct geographic variation in driving factors, and that over the target geography of the U.S. it seems to be dominated by factors other than increasing CO₂. See my powerpoint slide 1 for a zoom on the area of interest from the Zhu et al. 2016 paper.

"Zheng et al. (2017) confirmed the pattern..." should be modified to say "Zhen et al. (2017) confirmed the pattern of observed greening..." As written, and in the context of the previous paragraph, the current text suggests that Zheng et al. 2017 confirmed the pattern of greening being related mainly to increased CO₂, a factor which was not examined in their study.

Without the addition of the appropriate context in the preceeding paragraph (my notes 3, 4, and 5) the remainder of this paragraph could be misinterpreted to mean that the impacts of greening on air temperature are attributable back to a positive effect of CO₂ on greening.

The sentence "Greening has been observed globally but Chen et al. (2019) noted that China and India were exploiting it through land management changes" is misleading. Chen et al. (2019) shows that China and India are *causing* the observed greening in large areas by intentionally increasing the land area occupied by forests (China) and increases in harvested area in croplands, mainly due to multi-cropping, heavy fertilizer use, and expanded irrigation (China and India). It would be more accurate to say "Greening has been observed globally, but Chen et al. (2019) represent a disproportionate fraction of the total greening trend, accomplished through intentional increases in forest area (China), and expansion of multi-cropping, fertilizer use, and irrigation in croplands (China and India)."

Given the changes suggested in these two paragraphs, it raises the question of whether the section title (CO₂ and global greening) is itself misleading. That title wrongly suggests that all the content to follow is reflective of a direct influence of CO₂ on the observed global greening trends, when the literature presents a much more *nuanced* interpretation.

It would be worth noting in this section that the multiple remote sensing-based datasets for LAI changes have remarkable differences, especially when comparing the figure 2.1 reproduced from Zhu et al. 2016 with the MODIS-based data record from Chen et al. 2019 (their figure 1). That means it is difficult to place a lot of confidence in the attribution of changes in different regions to different factors. See my powerpoint slide 2 for a comparison of the four datasets used in Zhu et al. 2016 and Chen et al. 2019.

The authorship for the Haverd et al. 2020 citation is not correct in the References section for this chapter. The correct citation is: Haverd, V., B. Smith, J. G. Canadell, M. Cuntz, S. Mikaloff-Fletcher, G. Farquhar, W. Woodgate, P. R. Briggs and C. M. Trudinger 2020. Higher than expected CO₂ fertilization inferred from leaf to global observations. *Global Change Biology* 26(4): 2390-2402 DOI: <https://doi.org/10.1111/gcb.14950>.

A more recent and more comprehensive study (Keenan et al. 2023) reexamined multiple data constraints and concluded that the model used in Haverd et al. 2020 (CABLE) is outside the likely range of CO₂ fertilization effects, with higher CO₂ fertilization than indicated by multiple independent constraints. At the very least this more recent paper should be cited in the report, with text indicating that the central estimate from the TRENDY models is more likely to be in the range of observational constraints than is the CABLE result. As it stands the report suggests that the latest science indicates that most models have underestimated the fertilization effect, and the literature doesn't currently support that conclusion. See: Keenan, T. F., X. Luo, B. D. Stocker, M. G. De Kauwe, B. E. Medlyn, I. C. Prentice, N. G. Smith, C. Terrer, H. Wang, Y. Zhang and S. Zhou 2023. A constraint on historic growth in global photosynthesis due to rising CO₂. *Nature Climate Change* 13(12): 1376-1381 DOI: 10.1038/s41558-023-01867-2.

The attribution analysis in the Chen et al. 2024 paper is very sensitive to the choice of multiple regression vs. partial correlation (their figures 3 vs S7, respectively), and in both cases they seem to misattribute the significant increases in LAI in India and China to CO₂ rather than to changes in landcover and cropping practices. This kind of regression-based attribution is less informative than the model-based inference used for example by Zhu et al. 2016. The description of sensor calibration issues in this paper is also very cursory. It would probably be better to refer to the review paper of Piao et al. 2020 to support the claim of continued greening. Piao, S., X. Wang, T. Park, C. Chen, X. Lian, Y. He, J. W. Bjerke, A. Chen, P. Ciais, H. Tømmervik, R. R. Nemani and R. B. Myneni 2020. Characteristics, drivers and feedbacks of global greening. *Nature Reviews Earth & Environment* 1(1): 14-27 DOI: 10.1038/s43017-019-0001-x.

The section 2.1.2 (Photosynthesis and CO₂ levels) lacks coherence. It seems to be a collection of uncontroversial points about the ability of increased CO₂ concentration to stimulate plant growth and reduce water loss through stomates. Figure 2.2 seems intended to show that the current levels of CO₂ are not high compared to levels experienced in Earth's deep geologic past. A balanced treatment of the topic might call out the rate of change in CO₂ concentration from human-caused emissions, and how both that rate and the absolute levels diverge radically from rates of change and ranges of variability over the past hundreds, thousands, and hundreds of thousands of years. See my powerpoint slide 3 for an example set of figures that might be included.

The report suggests that the topics of CO₂ fertilization and increased water use efficiency are not adequately treated in recent IPCC reports. In IPCC's AR6 WG I report there is an entire chapter devoted to an assessment of carbon cycle processes (Chapter 5, Global Carbon and Other Biogeochemical Cycles and Feedbacks). This is precisely where one would expect to find information about CO₂ fertilization and water use efficiency. In the Executive Summary for the chapter is the following bolded headline: **Increases in atmospheric CO₂ lead to increases in land carbon storage through CO₂ fertilization of photosynthesis and increased water use efficiency (high confidence)**. The associated Executive Summary text includes reference to two figures, three chapter sections, and a cross-chapter box all dealing directly with this topic. The Executive Summary text also references the important topic of nutrient dynamics and their interactions with fertilization and ecosystem-climate feedbacks, a critical nuance of the interaction of CO₂ and vegetation productivity that is ignored in the report reviewed here.

Section 2.2.2 (Coral reef changes) has a strong focus on past examples of early investigations that are later updated. There is also a note about the difficulty of publishing negative results, with a quote that this pattern "is true across all of science". Given the objective of this report, I suggest a shift in tone away from complaints about the kinds of papers that do and don't receive media attention, and instead highlight the latest synthesis of results about the likely future of coral reefs. A relatively recent synthesis paper (Cornwall et al. 2021) in a well-respected journal (PNAS) based on collated data from 233 locations worldwide projects that 37% of reefs will stop accreting and thus fail to keep up with sea level rise by 2050 under the low emissions RCP2.6 scenario, and no reefs would be able to accrete at a rate to stay ahead of sea level rise by 2100 under either RCP4.5 or RCP8.5. This analysis finds that most of the loss of net accretion rate is due to bleaching events, and a minority of the impacts are due to "direct physiological impacts of ocean warming and acidification on calcification or bioerosion". C.E. Cornwall, S. Comeau, N.A. Korner, C.T. Perry, R. van Hooidonk, T.M. DeCarlo, M.S. Pratchett, K.D. Anderson, N. Browne, R. Carpenter, G. Diaz-Pulido, J.P. D'Olivo, S.S. Doo, J. Figueiredo, S.A.V. Fortunato, E. Kennedy, C.A. Lantz, M.T. McCulloch, M. González-Rivero, [...] & R.J. Lowe, Global declines in coral reef calcium carbonate production under ocean acidification and warming, *Proc. Natl. Acad. Sci. U.S.A.* 118 (21) e2015265118, <https://doi.org/10.1073/pnas.2015265118> (2021).

The study by Conolly et al. 2021 uses a linear regression approach to estimate the amount of variability in surface temperature trends that could be explained by total solar irradiance time series. Their approach is to first model the temperature trend with TSI as the dependent variable, and then compare the residuals from that fit to anthropogenic forcing factors. This is an obviously biased approach, and in fact a full paper was published the following year (in the same journal) laying out the erroneous methods and the faulty conclusions they produce (Richardson and Benestad 2022). Richardson, M. T. and R. E. Benestad 2022. Erroneous use of Statistics behind Claims of a Major Solar Role in Recent Warming. *Research in Astronomy and Astrophysics* 22(12): 125008 DOI: 10.1088/1674-4527/ac981c.

Section 3.2.2 (The carbon cycle relating emissions and concentrations). There is no new information or analysis presented in the chapter, and it relies heavily on Global Carbon Project information. Figure 3.2.3 is described as showing a seven-fold difference in the recent trend in land carbon sink estimates across land models, and this is given as a potential driver of uncertainty in future carbon cycle and climate change predictions. Variation among models is well known and is one of the reasons exercises like CMIP and GCP rely on multi-model ensembles when assessing the state of the science. The distribution of results looks fairly typical in this diagram, with a strong central value around 1.3 to 1.3.

The following sentence is misleading: "And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate." It is true that bringing global emissions immediately to zero would not reduce CO2 concentrations on short timescales. It is not true that this same scenario means no reduction in human influence on the climate. We must compare the zero emissions scenario to something, and the most meaningful comparison is the current trajectory of **increasing** emissions and the resulting **super-linear** increase in concentration. So by reducing emissions to zero we clearly avoid the expected increases in concentration and the expected increases in radiative forcing together with increased near surface air temperature and all the other expected consequences. So our change in action - going to zero emissions - does in fact have an immediate and growing human influence on the trajectory of the climate system. That is not an argument for reducing our emissions to zero tomorrow, it is just an argument for logic and clarity in how we talk about consequences of different actions.

Regarding the sentence "The practice of referring to unilateral U.S. reductions as "combatting climate change" or "taking action on climate" therefore reflects a profound misunderstanding of the scale of the issue." This makes the dubious assumption that when people refer to "combating climate change" or "taking action on climate" they are ignorant of the scale of the problem. Instead, it is probably more justifiable to assume that it is well-understood by most people who advocate for these local and national changes that the major emitters must act together to make a big difference in global emissions. It is at least worth considering, in the context of this report, that a path toward action at the necessary scale will require local and national action from a broad consortium, and that individual advocacy is a useful and necessary starting point for shifting the inertia of global policies.